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American Society of Parasitologists Henry Baldwin Ward Medal Acceptance Speech

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HENRY BALDWIN WARD MEDAL ACCEPTANCE SPEECH

Daniel R. Brooks

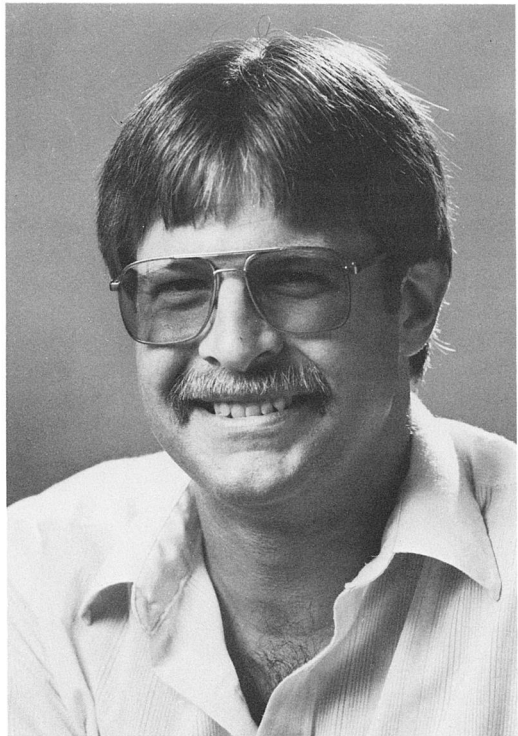
I am deeply honored to have been selected as this year's Henry Baldwin Ward medalist. The choice of an evolutionary biologist has special meaning, because it reaffirms the Society's support for basic biology, support which some have doubted in recent years. Singling out one person for praise will not alter funding patterns or hiring practices, but it is a positive step. At least now I can honestly tell my students that there will be a place for them in the Society which nurtured me.

In keeping with a quarter century of tradition, I will briefly discuss my roots, then recount something of my professional development, and finally tell you what I hope to do in the future to merit the praise you have bestowed upon me. I will try to make it interesting without actually lying.

I am descended from Scots-Irish Presbyterians who immigrated to North America in the 17th century, moved west and carved out a place in the wilderness, then settled down to lives of working hard and voting straight Republican. My upbringing followed the stereotyped pattern of my WASP background. I am an Eagle Scout, an honor shared by my father and four of my five brothers. I was a high-school All-American athlete and I graduated in the top 15% of my graduating class at Walt Whitman High School in Bethesda, Maryland.

In 1969 I accepted an athletic scholarship to the University of Nebraska, where I told a sports reporter that I had chosen Nebraska over the University of Pennsylvania because I did not want to be a scholar. I enrolled in an experimental program at the University of Nebraska called Centennial College, and when injuries curtailed my athletic career I focused my attention on Zoology, intending to go to medical school. Under the guidance of John Lynch, a herpetologist, I was steered away from pre-Medical courses and into interesting biology. Invertebrate Zoology with Carl Gugler and Parasitology with Brent Nickol instilled an abiding interest in invertebrate biology, and Lynch's Herpetology course turned me into a field biologist. In the spring of 1973, Mary Lou Pritchard accepted me as a graduate student, suggesting that I combine my interests in parasitology and herpetology by doing a survey of frog parasites in Nebraska. I began my graduate work in the fall of 1973 by learning what it meant to be a Nebraska parasitologist. I was working for Mary Lou, who had been trained by Harold Manter, who was a student of Ward himself. Furthermore, Nebraska was the place Ward began his teaching career. During my two years in the Manter Lab, I used Dr. Manter's microscope and worked at a bench with Ward's desk at my back. Such things can have a profound effect, especially after midnight.

The Nebraska parasitology group at that time was a large collection of Steinbeckian biologists. Those are the ones who tend to proliferate too much in all directions but are generally good company. Everyone did at least some field biology, and field biology was recognized as an honorable pursuit. In addition to Mary



Lou Pritchard, Brent Nickol and John Janovy, students included Dave Ashley, Dick and Shareen Buckner, Dave Oetinger, Alan Elkins, Rich Uznanski, Nelson Samuel, Bill Current, Steve Knight, Joan Decker, Pierre Daggett, Monte Mayes, and Guenther Kruse. Expatriates of previous academic generations would show up to reinforce the notion that there was something very special about Nebraska—people like G. Robert Coatney, Ellis Greiner, Paul Lewis, Betty June Myers, and Dave Becker. Monte Mayes and I may have been the most extreme of the group—not the best mind you, and with apologies to Pierre Daggett's tie-dyed lab coat. I smoked cigars and left butts all over the lab; Monte chewed tobacco and ruined trash cans with the residue. We both left carcasses in the lab over the weekend on more than one occasion, and I still have sweaty palms when I think about the time Mary Lou found an empty gallon jug of wine in the Manter lab before we had a chance to remove it. Monte and I were dubbed “serendipity research” because of all the non-thesis papers we wrote.

In 1974–1975 three important things happened to me. First, John Lynch forced me to take his graduate seminar on something called “cladistics.” John really knew me well, because at the end of the semester he told me that cladistics could not be done with parasites. In 1977 I published the first cladistic analysis for a

group of parasitic helminths. Second, I made a choice about my doctoral work, deciding to go work for Bob Overstreet. Among his many other accomplishments, Bob is the best morphologist of our day and certainly one of the all-time greats. In Mississippi I continued my interest in herpetology and used cladistics to look at the coevolution of crocodilians and their digeneans. I continued to proliferate in all directions and wrote a lot of papers, many of which still seem to have some socially redeeming features. I was fortunate to be a graduate student with Richard Heard, Tom Dearnoff, Tom Mattis, Mobashir Solangi, and Alan Fusco. And third, in the summer of 1975 I accompanied Tom Thorson of the University of Nebraska on a field trip to Colombia to begin studying the evolution of neotropical freshwater stingrays. Monte Mayes joined the expedition the next year and serendipity research put out nearly twenty papers over the next five years. Using cladistic analyses of the parasite groups we found, we were able to show that freshwater stingrays did not come from the Atlantic Ocean as previously thought, but were trapped from the Pacific by the uplifting of the Andes. The stingray study and the crocodilian study convinced me that there was something important in all this cladistics stuff.

In the fall of 1978 I arrived at Notre Dame to begin an NIH post-doc with Ted Crovello. Rich Uznanski, who had just finished with Brent Nickol at Nebraska, was my office mate. We worked on various aspects of computer applications in parasitology, Rich with population models and I with quantitative systematics. We argued for nearly a year about theoretical biology. The rest of the parasitologists at Notre Dame thought we were crazy, arguing about concepts when we could be homogenizing something. After all, as Vicki Funk of the Smithsonian has said, doing evolutionary biology is like putting together a jigsaw puzzle blindfolded and with half the pieces missing. But a major part of the Nebraska education is an appreciation of the conceptual basis of one's science—one understands first what it is that one is trying to do and why, then one begins using appropriate technology. When I left South Bend in the summer of 1979, I was convinced that the historical perspective had been neglected in studies of coevolution. By that time I had also met Ed Wiley who, along with David Hull, I consider to be one of the clearest thinkers and nicest people in the business. Ed was convinced that all of evolutionary theory lacked a principle of historical causality. I spent 1979–1980 at the U.S. National Zoo in Washington, D.C. At that time I was spending a lot of time alone, and one outcome was the discovery of the quantitative protocol for partitioning out the historical and non-historical influences in the evolutionary diversification of any group of organisms. The paper describing the method was published in 1981 and was couched in terms of looking at coevolution of hosts and parasites. It was dedicated to two of my brothers who had recently died. I also spent one day a week at Beltsville, learning a lot from Ralph Lichtenfels about nematodes. Ralph and Pat Pilitt bore the brunt of many of my growing pains at that time, and I felt more than a little guilty when Ralph developed an ulcer a few years ago.

I moved to Vancouver in July 1980, replacing James Adams, who had retired. A Nebraska parasitologist,

Hilda Lei Ching, eased my way into the local scene. In February 1981, Ed Wiley came to the University of British Columbia to present a seminar. We talked a lot about historical causality in particular, and evolutionary theory in general. One night the conversation turned to Ed's upcoming debate with a leading creationist. I had done some work on information theory, which uses equations taken from statistical thermodynamics. Together, Ed and I developed a line of reasoning designed to show how evolution was compatible with the Second Law of Thermodynamics using strictly biological examples. That the Second Law is the only physical law that had a sense of time, or historical causality, gave us a key insight to an expanded view of evolution, which has been published in part and which will be presented more fully in a book called *Evolution as Entropy* to be published by the University of Chicago Press in January 1986. The senior editor at Chicago, Susan Abrams, also edited Gerry Schmidt and Larry Roberts' parasitology text. Ten days ago I read the final page proofs in an alcove at Ralph Lichtenfels' lab in Beltsville, and it seemed very appropriate.

I have always felt that a good biological theoretician should have empirical research interests as well. I have tried to maintain some level of activity in empirical parasitology during the past five years, when I have been so heavily involved in theory. In this regard I have been greatly helped by my graduate students Richard O'Grady, Dave Glen, Susan Bandoni, and Cheryl Macdonald. Janine Caira was primarily responsible for my survival during the first year in Vancouver. My dean, Cy Finnegan, and my department head, Geoff Scudder, have been very supportive. I have also been helped substantially by my teaching. I really enjoy teaching, especially first year students, and I put a lot of effort into it. I try to convey a sense of my enthusiasm for the process of discovery and my apathy for the status quo—even my own.

Throughout the first dozen years of my professional life, I have been fortunate to have had support and friendly criticism from a number of parasitologists in addition to those I have already mentioned. I would especially like to thank John Mackiewicz, Bob Rausch, Reino Freeman, Bob Short, Gerry Esch, John Holmes, Tom Platt, Bill Font, Pat Muzzall, Ron Campbell, Murray Dailey, Al Bush, Danny Pence, Gerry Schad, Don Duszynski, Jeff Lotz, Larry Roberts, and Eric Hoberg. I am honored to have been able to talk with Horace Stunkard, Justus Mueller, Teague Self, Ray Cable, and Bob Coatney. I must also single out Gerry Schmidt, whose friendship and support I value more than he knows. He is one of the finest people around. I hope this is good enough for a free copy of his book.

The first seven years of my career were concerned mostly with empirical studies. When the results of those studies demonstrated insufficiencies in current theory, I spent five years doing mostly theory. Now I am at the beginning of a new cycle. There is the field of historical ecology and a new theory of evolution to be tested and played with like new toys. With the addition this fall of two new graduate students who study parasitic copepods on elasmobranchs, we should be able to compare the evolution of ecto- and endoparasites of the same hosts using the same analytical technique. Every field biologist will tell you that there is no end

to the interesting things you can find when you dive into a new tide pool or turn off the road into new jungle. And there will always be the Nebraska influence. I took two students to the Sea of Cortes in December 1985. We stayed at a small field station run by a consortium of small schools in southern California. Their student handbook stated that their “Baja Experience” was to be conducted by the “Janovy method.” I knew exactly what to do. So, I am going to continue to be busy, and I will probably continue to be controversial.

I must finish with a few words about what it has been like to be a controversial young scientist. It has been my most direct lesson in John Janovy’s distinction between things done for money and pride and those done for love and joy. There is no money or pride in being controversial. Granting agencies are afraid to risk *their* money and pride on you, and faculties who want to hear you present your new ideas do not want you as a colleague. There may be money and pride if your controversial ideas become dogma, but that will not sustain you during those lonely days, and it will disappear as soon as some bright-eyed graduate student finds the essential flaw in your version of the perceived

truth. Only a love of discovery and joy in your work can sustain you. For those younger people in the audience who rightly realize that they are probably smarter than I am, I say that becoming controversial is not an easy way to gain recognition. You must be more than smart—you must be brave and resolute, sustained by an inner love of what you are doing. You must have people to love and support you, even if they cannot go the full distance with you. The people I have mentioned above are not people who necessarily agree with all, or even most, of what I say. But they are people who respect my trying and can laugh. And I think, most importantly, you must take what you do seriously without taking yourself seriously. Do not lose the ability to laugh at yourself. If you take yourself seriously and recognition does not come in your lifetime, you will die unhappy. And if recognition does come in your lifetime, you will be trapped into believing the myth you have created. You will be defending the banner rather than leading the charge. Science is a never-ending story in which the flow of discovery is reality and our theories are only gauges that help us monitor some part of the flow.